Department of Energy
Washington, DC 20585

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Mr. Joseph J. Holonich, Director
Repository Licensing & Quality Assurance
Project Directorate
Division of High-Level Waste Management
Office of Nuclear Material Safety
and Safeguards
U. S. Nuclear Regulatory Commission
Washington, D. C. 20555

Dear Mr. Holonich:

Enclosed are the U.S. Department of Energy's (DOE) comments on the Center for Nuclear Waste Regulatory Analyses (CNWRA) report, CNWRA 91-011, entitled "Sorption Modeling for High-Level Waste Performance Assessment: a Literature Review." Appendix A of DOE's comments contains additional references that DOE recommends the CNWRA use to supplement its reference list in report 91-011.

If you have any questions, please contact Corinne Macaluso of my staff at (202) 586-2837.

Sincerely,

Linda J Desell

Linda J. Desell, Chief
Regulatory Integration Branch
Office of Systems and Compliance

Enclosure: As Stated

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cc:
C. Gertz, YMPO
T. J. Hickey, Nevada Legislative Committee
R. Loux, State of Nevada
D. Bechtel, Las Vegas, NV
Eureka County, NV
Lander County, Battle Mountain, NV
P. Niedzielski-Eichner, Nye County, NV
W. Offutt, Nye County, NV
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REVIEW COMMENTS ON "SORPTION MODELING FOR HIGH-LEVEL WASTE PERFORMANCE ASSESSMENT: A LITERATURE REVIEW" By David R. Turner, Center for Nuclear Waste Regulatory Analyses, San Antonio, Texas

GENERAL COMMENTS

The subject report provides a valuable start on creating a reference base for sorption studies and modeling of relevance to the Yucca Mountain Site Characterization Project. In this respect the Center for Nuclear Waste Regulatory Analyses (CNWRA) is to be commended. Even so, some sort of critical evaluation would be useful to put the problems in perspective. Because of the breadth of material presented, the overview on sorption modeling and coupled sorption/transport modeling is, by necessity, superficial. Consequently, the document cannot be used as an explanatory introduction into the sorption or transport modeling field. However, the document is useful as a reference source, especially for references to coupled sorption/transport models and for references to recent work done on sorption of relevant radionuclides (section 5.5). As indicated by some of the ensuing comments, the discussion of the literature would benefit from a more careful analysis of the papers and better understanding of the chemistry and models.

DOE agrees that both mechanistic and empirical approaches have their place when modeling the effect of sorption on radionuclide transport. This is similar to the strategy proposed by Study Plan 8.3.1.3.4.1.

The use of coupled models to identify critical data needs and to simplify the description of the chemical system is rational, and the goals are not unreasonable. However, it is unlikely that the proposed approach will achieve these goals. It is proposed to take a complex computer program and make it even more complex, without having given adequate consideration to the types and quality of chemical data that will be required.

The subject report states that one of the objectives of the work it describes is the selection of a single computer program for ". . . addressing increasingly complex and realistic problems" involving sorption (and, necessarily, other chemical processes) coupled to advective-dispersive transport of solutes. It may be wiser, to select two or three different programs and to use them simultaneously. Such "benchmarking" would allow identification of the most useful features of each program, objective assessment of the efficiency of each program, and identification of any numerical problems. Concentration on a single program will not produce this information. The paper by Read and Williams (1987) in the Additional References provides an example of benchmarking using coupled programs.

The report does not appear to recognize that each of the coupled computer programs considered (TRANQL, FASTCHEM, CTM, CHEMTRN, CHMTRNS) is a research tool, in the sense that the main purpose of the program is to assist in understanding the chemical and physical processes active in transport of chemically reactive solutes. It is acknowledged generally that the complete body of chemical data needed for large scale applications (as in performance assessment) of these and similarly constituted programs probably will never be available. Therefore, the best uses of such programs are to identify critical areas that need further investigation (e.g., important sorption mechanisms) and to provide bounding values of "lumped parameters" that can be used in performance assessment calculations. These uses are mentioned in the discussion in Section 6.3, "Recommendations". However, the report implies that the chosen program, CTM, will be expanded and eventually used to make performance assessment calculations. Given the lack of knowledge of sorption mechanisms and the lack of thermodynamic and kinetic data even for presently identified mechanisms, it does not seem likely that such an undertaking will be successful.

The report could be strengthened in the general area of chemistry. For example, no mention is made of the use of techniques, such as various types of spectroscopy, that can are being used for experimental studies of chemical reactions at surfaces. There is no discussion on the roles played in sorption by different solution species and their modes of bonding to the surface of a mineral. Yet, this kind of information is needed to construct and run a numerical simulator of whatever sorptive process is active. The chemical information needed for a complete description of any sorptive process is difficult to obtain; this may explain why little progress has been made so far in the chemical modelling of sorption. Additional references are included on methods for investigation of chemical reactions at surfaces, drawn from a recent American Chemical Society (ACS) symposium.

In addition, the report ignores a large body of literature concerned with <u>analytical</u> solutions of problems of advective-dispersive transport coupled to sorption. Such solutions are often useful for making rough, "scoping" calculations and for verification of numerical solutions. Some references to this body of work are included in the Additional References. Other additional references are provided to modelling and database efforts described at another ACS symposium, and to various other papers on numerical modelling of reactive chemical transport.

In most geochemical modeling codes, surface complexation and ion exchange are treated as "aqueous" processes and are decoupled from the precipitation or dissolution of surface-active phases. Thus, there is no simulation (aside from solid-solution models) of the creation or destruction of phases that surface complex or ion exchange. For modeling the long term behavior or sorbing minerals at Yucca Mtn., coupling sorption to dissolution/precipitation is necessary. The author does not address this problem.

The section on modeling coupled processes (Section 4.3) might have included a description of codes such as PHREEQM (Appelo, C.A.J. and Willemsen, A., 1987. Geochemical calculations and observations on salt water intrusions, I. A combined geochemical/mixing cell model. J. Hydrol. 94:313; Appelo, C.A.J. et al. 1992. PHREEQM-PHREEQE in a mixing cell flowtube. p. 201-204. in Water-Rock Interaction. Kharaka, Y. and Maest, A. (eds), Balkema, Rotterdam) used for modeling flow and surface chemistry. There is also no mention of the approach taken by Peter Lichtner (Lichtner, P.C. 1985. Continuum model for simultaneous chemical reactions and mass transport in hydrothermal systems. Geochim. Cosmochim. Acta 49:779; Lichtner, P.C. 1988. The quasi-stationary state approximation to coupled mass transport and fluid-rock interaction in a porous medium. Geochim. et Cosmochim. Acta 52:143) and Richard Knapp (Knapp, R.B. 1989. A Lagrangian Reactive Transport Simulator with Successive Paths and Stationary-State: Concepts, Implementation and Verification. Lawrence Livermore National Laboratory UCRL-100952. Richard Knapp's review of reactive solute transport (Knapp, R.B. 1988. Reactive Solute Transport: A Review of Current Concepts, Methods and Computer Models. Lawrence Livermore National Laboratory. UCID-21516) is a very useful comparison of modeling approaches and codes that should be cited.

By comparison with the available literature on sorption of radionuclides, it appears that the CNWRA has used some selection criteria as to what to include. These should be clearly stated. For example, the literature on sorption of radionuclides during the treatment of nuclear wastes is not included, even though some of that information may be relevant. Several studies of sorption by sedimentary rocks are similarly missing, even though some of the same, or similar, minerals are present in both sedimentary and igneous rocks. The scope of the review by CNWRA apparently did not include such topics as isotopic fractionation, nor the effects of radioactive decay chains on sorption elements that are available for sorption and desorption. Finally, some of the significant papers on modeling, or the basis for the character of breakthrough curves, are not cited. However, to include all these topics, as well as providing annotations to all the papers in Appendix B, the ones included here as suggestions, and others that postdate the data base from which the suggestions were extracted, would greatly increase the effort.

SPECIFIC COMMENTS

1. DOE agrees with and would emphasize the following statements made in the Executive Summary:

> "If sorption modeling is to take advantage of the computational simplicity offered by empirical strategies, empirical models must be adapted to a variety of physical and chemical conditions."
>
> "Faced with this lack of data, the application of mechanistic models reduces to a curve-fitting exercise, losing the advantage of a theoretical basis, and increasing the likelihood of non-unique solutions."
>
> "A difficulty encountered in using field studies for evaluating contaminant migration is the uncertainty inherent in characterizing any natural geologic system."
>
> "Evaluation of sorption at Yucca Mountain must be considered in the context of these activities in order to limit the amount of experimental effort required to provide the necessary data for modeling radionuclide migration."

2. Page 1-1, paragraph 2, line 6-7: It would be useful to cite and reference the proposal for a multiple-barrier design.

3. DOE fully supports the following statement on page 1-2, paragraph 2:

> "Simplification of the processes involved in the attenuation of reactive solute transport is necessary in order to make the problem tractable."

4. Page 2-2, paragraph 3: In the discussion of surface site density, there is no mention of total site density versus the density of available sites. In a real rock, not all the mineral surfaces are necessarily exposed to the fluid phase. Similarly, the available surface area may not be the same for all solutes depending on size, charge, etc. In the discussion of cation exchange capacity, there is no mention of site energy distributions in ion exchangers or of selectivity coefficients. Nor is there any discussion of how these parameters might be obtained.

5. Page 2-3, paragraph 2: It would be useful to discuss the distinction between the PZC and the IEP (Isoelectic Point). Also it is not clear at the present time that the surface potential has less of an effect on the simple cations versus the hydrolyzable metals as is stated.

6. Page 2-5, first line: The statement that "Adsorption is strongly dependent on solution pH" should be softened somewhat. For instance, ion exchange reactions seem to be much less sensitive to pH than surface adsorption reactions. If the statement refers only to surface adsorption reactions then it is probably applicable.

7. Page 2-5, paragraph 2, lines 5-10: Uranium complexes with carbonate are anionic (e.g., $U(CO_3)^{4-}{}_4$, $UO_2(CO_3)^{2-}{}_2$), which largely explains their enhanced mobility relative to U^{4+} and $UO^{2+}{}_2$. The explanations advanced here are obscure.

8. Page 2-5, next to last line: The term "organo-metallic" refers strictly to compounds that contain a metal-carbon bond. The complexes under discussion here do not contain metal-carbon bonds; the complexing is achieved usually by metal-oxygen bonding. Better terminology here would be "complexes of metals with polydentate (or chelating) ligands".

8\. Page 2-7, lines 3-6: The sorption-concentration relationships are defined if the sorption or exchange isotherm is known for each competing ion. The term "nonuniquely defined" is an oxymoron.

9. Page 2-8, paragraph 3: The discussion of ionic strength effects is somewhat lacking. For example, in addition to the effects that ionic strength might have on activity relationships, there clearly are also the simple mass action effects in ion exchange reactions. In surface adsorption processes, Hayes et al. (1988) have used experimentally observed ionic stength effects in distinguishing inner-sphere from outer-sphere complexation.

10. Page 2-8, last paragraph: The importance of the mineralogy of the substrate to sorption reactions is discussed. In reality, the surface composition and surface structure of minerals are more likely to be important to adsorption reactions. This composition and structure may be quite different from the bulk composition and structure of the underlying mineral phase.

11. Page 2-9, paragraph 2: The discussion of water/rock ratio effects is somewhat confusing and the section on Fluid Flux and Fluid Mixing seems incomplete, inasmuch as it does not mention desorption or desorption rates. Rates in both the forward and backward directions are important for equilibrium considerations.

12. Page 2-10, last paragraph: The statement that "Because aqueous speciation cannot be addressed by empirical approaches, these methods may be inadequate to model the interactions that occur during multispecies transport (Higgo and Rees, 1986)" depends on speciation kinetics. If speciation kinetics is sufficiently fast, there should be no problem.

13. Page 2-11, Section 2.3.1.1, Eqn. 2-2: Such manipulations as performed in equation 4-4 are not always mathematically permissible. In this case it leads to the conclusion that the retardation factor is always the same, regardless of position on the breakthrough curve. Indeed it is contrary to either sharpening or spreading of the breakthrough front. A discussion of the derivations by Hiester and Vermeulen (1952) would also make it clear that the retardation takes place almost entirely within the "exchange zone", as the infiltrating solution contacts minerals that are not already saturated with respect to the advecting radionuclide rather than in front of or behind that zone. Equation 4-4 by itself does not convey that meaning.

14. Page 2-11, last paragraph: Meaning of the following sentence is unclear: "Competitive sorption can only be modeled by approaching each species individually, and the technique does not allow for interaction between solutes."

15. Page 2-11, next to last line: CHEMTRN uses finite <u>differences</u>.

16. Page 2-15, section 2.3.1.4: Clearly, S and b have <u>exactly</u> the same meaning here as they do in Eqn. (2-9). However, KDR is drastically different from KLa. In Eqn. (2-11), KDR must be < 0 in order for S to be bounded as C approaches 0; this then allows E to be real in Eqn. (2-12). However, KLA in Eqn. (2-9) is > 0. The discussion gives no clue as to how the two K's are related to energies of sorption, nor to the basis for Eqn. (2-12). Most importantly, no basis or assumptions leading to Eqn. (2-11) are stated.

17. Page 2-15, last paragraph: Sorption-desorption hysteresis can be caused by other processes (e.g., non-reversible adsorption, chemisorption) besides kinetics. The statement ". . . sorption is dealt with as a separate process . . . " is obscure: separate from what?

18. Page 2-19, last paragraph: Activities of sorbed phases are often estimated by their mole fractions (the ideal solution approximation) or equivalent fractions.

19. Page 2-21, paragraph 5: It would be useful to provide a definition of the potential function.

20. Page 2-22, paragraph 1, last line: It is stated, "The complexation approach is valid for mono- and multivalent cation and anion exchange, limited only by data availability." It is

suggested to insert the word "simple" be in front of both cation and anion. Competitive effects cannot be addressed successfully by the theory as it is formulated at present.

21. Page 2-22, paragraph 2, sentence 3: It is stated that "The ability to account for variable physical-chemical conditions gives surface complexation models a flexibility of application based on theoretical considerations." Use of the word "theoretical" in this context may be overstating the case. Surface complexation models still contain a substantial amount of empiricism.

22. Page 2-22, eqn. 2-30: This equation doesn't make sense. It says that the number of sorption sites increases linearly as the concentration in solution increases.

23. Page 2-23, paragraph 2: the reason for discussion of Cd complexation is unclear.

24. Page 2-25: In discussing the triple layer model, the modification introduced by Hayes and Leckie (1986) in which metal ions are allowed into the inner o-plane in addition to hydrogen ions, etc., is not included. This comment also applies to Figure 2-3.

25. Page 2-28, paragraph 3: It is important to emphasize that the "characterization" of silicates, carbonates, aluminosilicates, and whole-rocks, in terms of triple layer models involves only one type of site, with only a single energy, on each mineral. This is likely an unrealistic expectation of the model. Further, the meaning and significance of triple layer characterization of whole-rocks is unclear.

26. Page 2-33: In the discussion of advantages and disadvantages of the various sorption models, a statement is made which is critical to the overall discussion. The statement is: "Coupling of these approaches with transport codes may lead to excessive amounts of computational time and memory requirements (see Section 4). In addition, much of the data necessary for rigorous application of these models are frequently either unavailable or poorly constrained at present (Kent et al., 1988; Siegel et al., 1989; Hayes et al., 1989; Serne et al., 1990; Meijer, 1990). Much of the data are only available for pure or synthetic minerals, and extrapolation to natural solid solutions and composite materials is problematic (Kent et al., 1988; Meijer, 1990)." Given the potential impact of this statement on the modeling of sorption reactions in a repository environment, it would be useful to elaborate on the statement: First it is suggested to add the words "complex electrolytes," before "natural solid-solutions" in the last sentence. Second, one could elaborate with answers to questions such as: Are there examples in the literature of successful application of double or triple layer models to transport in complex natural systems? Are there fundamental flaws in the electrostatic models? Are there examples of problems encountered in the application of the models to systems other than single substrate/non-sorbing electrolyte systems?

27. Page 3-1, paragraph 3: Discussion of the solid-phase characterization methods does not include any discussion of surface characterization techniques such as LEEDS, EXAFS, AFM, STM, ESCA, etc.

28. Page 3-2, 1st full paragraph: Typically the water used in batch sorption experiments is preconditioned, but seldom, if ever, is it left in contact with the rock long enough to come to chemical equilibrium. The term "equilibrated" in the third line is misleading. Later in the paragraph the statement that "concentrations must not be so undersaturated as to promote dissolution of the solid phase(s)" is very puzzling. If the discourse up to this point means that a trace of an element that was not present in the rock was being used as a spike, there will be no solid phase present that contains that element, and no need to limit the undersaturation on that account.

29. Page 3-9: A discussion of the usefulness of uranium and thorium series disequilibrium studies might have been added to this section on analogues.

30. Page 4-1, Eqn. 4-1: This equation appears to ignore chemical diffusion, unless it is intended to lump it together with hydrodynamic dispersion. If so, this should be stated.

31. Page 4-6: EQ6 also includes one dimensional flow tube capability, in this case focused on the chemistry of the solution as it moves along the tube and reacts with the enclosing rock, rather than on the hydrology. This capability purportedly includes solubility, kinetics, solid solutions, and temperature variability, all of which are discussed in other sections of this report, although EQ6 does not explicitly include sorption (one might be able to approximate some forms of sorption through use of the solid solution option). Some mention of this would seem appropriate.

32. Page 4-12, first complete paragraph: One-step coupling yields a solution that simultaneously satisfies the numerical analogs of the transport equations and mass action equations describing chemical equilibria. Two-step coupling yields, in one step, a solution to the transport equations and, in the other step, a solution to the chemical equations. In the two-step case, there is no guarantee that the transport solution satisfies the chemical problem, and vice versa. In particular, the two "solutions" may differ considerably if precipitation occurs. Agreement might be achieved within a time step by iterating between the two solution procedures. However, this is not done in TRANQL. The statement that ". . . the coefficient matrix must be reformulated for each iteration . . ." is puzzling because the coefficients in question are not identified.

If by "coefficients" are meant the elements of the Jacobian matrix in the Newton-Raphson solution procedure used by CHEMTRN and CHMTRNS, then the statement is not correct. The statement that ". . . two-step coupling . . . uses computer resources much more effectively" is not substantiated by any direct comparison of one-step and two-step codes. The statement that ". . . Yeh and Tripathi (1989) show that only a two-step approach will be able to model larger two- and three-dimensional problems . . . " is a distortion of Yeh and Tripathi's conclusions. Furthermore, these conclusions were conjectural and, again, not based on a direct comparison of one-step and two-step codes.

33. Page 4-12, second complete paragraph: The only reference to a specific database is to the FASTCHEM database (Krupka et al., 1988). No mention is made of the EQ3/6 database developed by T. Wolery and associates at LLNL, which is the premier database for chemical modelling of Yucca Mountain, nor the Nuclear Energy Agency thermochemical database.

34. Page 4-13, second complete paragraph: The excessive run times of the TRANQL code are not typical of other reactive chemical transport codes (either one- or two-step types) such as CHEMTRN, CHMTRNS, HYDROGEOCHEM, THCC, etc.

35. Page 4-16, last paragraph: The statement that the CHEMTRN code is ". . . more cumbersome than the two-step codes . . ." is technically indefensible. The statement, "Due to the one-step coupling, however, storage limitations will become a problem in extending the model to multiple dimensions", is misleading because problems of storage limitations that arise from increased dimensionality and more complex methods of solution will be common to both one-step and two-step methods.

36. Page 4-17, lines 6-7: Several well-developed data bases already exist. It should be a relatively simple matter to write a program that restructures these into a format usable by any other program, or to modify the input statements within such a program to read the existing bases.

37. Page 4-17, first paragraph, last sentence: The statement that ". . . a database would have to be developed and modified for extensive use of this code" is incorrect. The CHEMTRN code (and its derivatives CHMTRNS and THCC) can use any existing database. The same comment applies to p. 4-18, first paragraph, lines 5-7.

38. Page 4-17, last paragraph: The statement that the two-step approach used in the CTM code allows it ". . . to solve for reactive solute transport [sic] more efficiently than the one-step approach used in CHEMTRN and CHMTRNS" is conjectural. It should be removed unless it can be substantiated by reference to a published comparison of the performance characteristics of the three codes.

39. Page 5-1, first line in section 5: The author should have used "potential" instead of "proposed" when referring to Yucca Mountain as a site for proposed high-level waste disposal repository.

40. Page 5-8, second paragraph, line 3: In many, if not most cases, the fractures are only partially lined with secondary minerals. The statement here and others later give the impression that the solution in the fractures will contact only the secondary minerals, and hence not the matrix quartz and feldspar.

41. On page 6-1, paragraph 3, the strategy presented in Meijer (1990) has been superseded by the "minimum Kd" strategy presented in Meijer (1992). The latter reference may not have been available to the author for inclusion in this review. However, it does address the reservations regarding the Meijer (1990) strategy as expressed in the review.

42. Discussion of the electrostatic sorption models is somewhat confusing. The description of the constant capacitance (CC) model as a special case of the diffuse layer (DL) model and the representation of the DL and CC models in Figures 2-1 and 2-2 are misleading. Westall (1986) provides a more rigorous comparison of the two models in which the electrostatic energies of the surface are formulated in terms of the capacitance. He shows that the DL and CC models both are limiting cases of the Stern Model, which contains an inner compact layer and an outer diffuse layer. The total capacitance of the interface is given by the equation for two capacitors in series:

$$1/C_T = 1/C_1 + 1/C_d$$

At low ionic strength, the diffuse layer capacitance C_d is much less than that of the compact layer C_1 and the surface approximates that of a diffuse layer. At high ionic strength, the properties of the compact layer dominate the behavior of the surface.

43. Equation 4-4 should apply to both linear and nonlinear isotherms (i.e. where dS/dC is not a constant).

44. The discussion of dual-porosity flow and matrix diffusion (Section 4.1.2) is not complete. The author fails to mention the large number of papers produced by I. Neretnieks' group that deal with these phenomena. He also missed Erickson et al (1986) who provided a framework for the chemical aspects of fracture/matrix coupling that leads to numerical criteria for determining when matrix diffusion can be represented by an equivalent porous medium or a semi-infinite matrix approximation. Finally, the placement of the discussion of two-region kinetic models in the section devoted to the Local Equilibrium Assumption is confusing; it really belongs in the discussion of matrix diffusion (Section 4.1.2). In fact, van Genuchten's model was the basis for the dual-porosity model of NEFTRAN.

45. The discussion of coupled hydrogeochemical codes is not complete; it does not include the HYDROGEOCHEM code (Yeh and Tripathi, 1988).

46. Page 5-15, Section 5.5.2.1: It is not the presence of atmospheric CO_2 per se that increases the solubility, but of the complexing ligand, CO_3^{2-}. The main point is that the impact of complexing ligands depends strongly on the concentration of that ligand. This point is not discussed in the report. Specifically, the concentrations of phosphate and fluoride, whereas detected in some analyses of waters present near Yucca Mountain, are very low.

47. Page 5-15, last paragraph: The sentence beginning, "In the presence of atmospheric CO_2 . . ." is misleading because it implies that the presence of CO_2 is required for complexation of uranium. This is not true. The following two statements are true: (1) uranium forms complexes with a variety of ligands, and (2) at higher pH in bicarbonate solutions uranium forms a variety of of carbonato and hydroxycarbonato complexes. The sentence beginning, "Complexation is also significant . . ." is misleading. The degree of complexation with phosphate, fluoride, carbonate, or other ligands depends on the relative concentrations of the ligands and the pH of the solution.

48. Page 5-23, last line: This sentence suggests that the author has a misunderstanding in respect to the meaning of figure 5-9. Yes, the Eh at which Tc(IV) oxidizes to Tc(VII) occurs at a higher Eh at lower pH, but essentially everything else also shifts to higher Eh at the same time. It is not obvious that the range of Eh values over which Tc can exist in the tetravalent state changes much. If it does it would be due to the interplay of the slopes of the dominance fields of Tc in the di-, tetra-, and heptavalent states and the lower stability limit of water, and not merely the relation between the +4 and +7 states.

49. Page 6-2, last sentence to top of page 6-3: This conclusion seems unduly pessimistic; a modular approach could include all of these processes with existing technologies.

50. Page 6-3, third complete paragraph: It has not been demonstrated by direct comparison of codes that ". . . a two-step approach is more efficient".

51. References: The following references are defective in the sense that not enough information is given to enable the reader to obtain the original text:

Burns, R. O. et al., 1989
Conca, J. L., 1990
Hobart, D. E., et al., 1989
Marley, N. A., et al., 1990
Meijer, A., I. Triay et al., 1989
Pabalan, R. T., and K. S. Pitzer, 1990
Read, D., and T. W. Broyd, 1989
Ticknor, K. V., and B. Ruegger, 1989
Wolery, T. J. et al., 1990
Yang, I., 1991
Yeh, G. T., and G. P. Gwo, 1990

It is noted that of the 320 cited references, only 159 are annotated in Appendix A. The other 161 should be accorded equal status. The purpose of Appendix B is obscure. If these references are not worthy of mention in the text, there seems to be no point in recording them.

ADDITIONAL REFERENCES:

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Ames, L. L., and Rai, D. (1978). Radionuclide Interaction with Soils and Rocks, Vol, 1, Processes of Radionuclide Mobilization and Retention (title approximate), EPA 520/6-78-007A

Apps, J. A., Carnahan, C. L., Lichtner, P. C., Michek, M. C., Perry, D., Silva, R. J., Weres, O., and White, A. F. (1983). Status of Geochemical Problems Relating to the Burial of High-Level Radioactive Waste, 1982, U. S. Nuclear Regulatory Commission, NUREG/CR-3062, LBL-15103, Lawrence Berkeley Laboratory, Berkeley, CA

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Barney, G. S. (1984). Radionuclide Sorption and Desorption Reactions with Interbed Materials from the Columbia River Basalt Formation, Geochemical Behavior of Disposed Radioactive Waste, American Chemical Society, pp. 3-23

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Bruehl, H. K. (1986). Results of Studies of Sorption and Desorption of Radionuclides on Sedimentary Rocks from Gorleben and the Konrad Mine, Zeitschrift der Deutschen Geologischen Gesellschaft, Vol. 137, pp. 121-136

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Ching-Kuo, D. H., and Langmuir, D. (1985). Adsorption of Uranyl onto Ferric Oxyhydroxides: Application of the Surface Complexation Site-Binding Model, Geochimica et Cosmochimica Acta, Vol. 49, pp. 1931-1942

Davis, S. N. (1984). Research Needs Related to Element Migration and Fixation, Workshop on Fundamental Geochemistry Needs for Nuclear Waste Isolation, Los Alamos National Laboratory, CONF-8406134, pp. 135-136

Delegard, C. H., Barney, G. S., and Gallagher, S. A. (1984). Effects of Hanford High-Level Waste Components on the Solubility and Sorption of Cobalt, Strontium, Neptunium, Plutonium, and Americium, Geochemical Behavior of Disposed Radioactive Waste, American Chemical Society, pp. 95-112

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FY 1994 Budget
Regulatory Policy & Requirements Branch

FY 1994 Base Case

Activity	Dollars
Total--	2,665K
Continue to issue and update regulatory guidance documents for MRS, repository and transportation projects. Update the documents to facilitate meeting repository and MRS milestones.	375K
Develop Environment, Safety and Health 5 Year Plan.	250K
Coordinate NEPA compliance activities for all projects. Review/approve all Operations Office NEPA-level determinations and OCRWM Environmental Assessments (EAs) and Environmetal Impact Statements (EAs).	565K
Develop OCRWM input to proposed regulatory initiatives from NRC, EPA, DOT, and DOE to ensure implementation by repository, MRS, and transportation projects. Continue Environment, Safety and Health (ES&H) coordination for OCRWM regulatory documents and manage issue resolution activities for all OCRWM projects.	725K
Continue OCRWM self-assessment program regarding environmental protection, radiological and non-radiological public safety, and occupational safety and health. Conduct OCRWM QA Management Assessments.	750K

FY 1994 Decrement (-15%)

Activity	Dollars
Total--	2,265K
Delay issuance of regulatory guidance documents for repository, MRS, and transportation projects, resulting in adverse impact on ability to meet the MRS milestone for EA preparation.	320K